of the grain of wheat. These bodies may be easily seen in the coarser kinds of bread; and drawings were exhibited giving their character before being taken into the body—which corresponded precisely with the specimens obtained from cholera patients. The presence of these bodies was also accounted for.—3d. Bodies of lighter colour and more delicate. These were found to agree precisely with grains of starch from wheaten flour; and drawings of both were exhibited. From these observations, Mr. Busk was led to conclude that the fungous theory derived little or no support from the facts brought forward by the miscroscopists of Bristol: although he did not deny that subsequent investigation might lead to the discovery of something which had hitherto eluded observation."

Since the announcement of Mr. Brittan's discovery, a series of microscopical observations has been undertaken in Edinburgh by Dr. J. H. Bennett and Dr. Robertson, in order to test the accuracy of his conclusions. As these investigations are still in progress, we cannot yet present our readers with the details: but we are authorized to state—

1st. That the "annular bodies" described by Messrs. Brittan and Swayne have been frequently found in the "rice water" evacuations of cholera patients.

2d. That, in the majority of cases examined, they have not been seen.

3d. That they have never been found in the moisture condensed from the atmosphere surrounding cholera patients in Edinburgh, nor in the water used in the city.

4th. That they cannot be confounded with starch globules, as they resist the action of iodine.

5th. That cholera evacuations often exhibit other *and less equivocal vegetable* bodies, such as sarcina, forulæ, and branched fungi.

6th. That a magnifying power of 250 diameters is quite sufficient to exhibit the "annular bodies" when present.—*Monthly Journal*, Nov. 1849.

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53. *Fungoid Origin of Cholera.*—Dr. Nicholas Parker, Lecturer on Microscopical Pathology, at the London Hospital, in an interesting paper in the *Lond. Med. Gaz.*, Oct. 1849, observes:—

"The doctrine that certain epidemic and contagious diseases arise and become diffused through the agency of minute living organisms, is by no means a new theory. With various modifications, it has been more or less prevalent for centuries. But it had become well nigh exploded until attention was again directed to it in our own time by the able writings of Holland and of Henle. Various arguments have been adduced in its support; the more important are of two kinds, negative and positive.

The negative arguments are:—

1st. The insufficiency of known physical causes to account for the origin and spread of contagious diseases.

Thus, to explain the causes of cholera, unwholesome food, and impure water, peculiarities of living, of climate, and of geographical situation, changes in the physical properties of the atmosphere, of its temperature, its hygrometric state, its electrical condition, and circumstances connected with the geological formation of countries, have in turn been invoked and abandoned. The Hindoo, in the sultry plains of Bengal, who subsists chiefly on vegetable food, and the Russian, inhabiting a climate where the thermometer is many degrees below zero, and living almost exclusively on animal diet, have nearly equally suffered. Hill and plain, dry and moist situations, valley and mountain (as at Landour, 8000 feet above the level of the sea) have all suffered, though in very unequal proportions. We cannot, in any of these circumstances, find a clear and sufficient explanation of the cause of the disease.

2d. Chemical analysis cannot detect the presence of any deleterious agent in the atmosphere to which these diseases seem fairly referable; and yet cholera, like typhus, influenza, and the exanthemata, is clearly dependent upon some wide-spread general cause. For it appears spontaneously in a district which had been previously healthy; it attacks simultaneously, or nearly so, a number of persons; it is migratory in its course, passing from one locality to another,

and that frequently at a considerable distance; it attacks individuals of both sexes, of every age, and of every constitution.

Epidemic diseases, then, cannot be referred either to changes in the known physical properties of the earth, air, water, or food, or to chemical alterations of the ingesta and circumfusa (for the ozone theory seems to be quite disproved by the remarks in the "Medical Gazette" of last week); and hence they have been thought by some to be dependent upon a minute living organism, an animated principle of contagion. The following are some of the more important positive arguments by means of which this view has been attempted to be supported:—

1st. The increase of these diseases is promoted, retarded, or arrested by means that promote, retard, or arrest the growth and development of such minute living organisms. Along the courses of rivers, more particularly where they form a delta, rarely at their source, in alluvial basins and in damp situations generally, wherever large masses of animal matter, in a state of decomposition, are allowed to accumulate—as about sewers, and those plague spots, the metropolitan graveyards—there do we find cholera, typhus, and their congeners, to be rife, and there do they commit the greatest ravages: and these are precisely the circumstances which favour the increase and development of the lower organized forms.

2d. The poison which produces many of these diseases possesses a capability of increase when received into the body, reproducing itself at the expense of foreign organic matter, so that the effect produced is not at all in relation to the frequently small quantity of poison taken into the system. Like yeast, which is known to be a minute fungus, torula cerevisiæ, these poisons increase by assimilation. Just as a small quantity of yeast will suffice for the fermentation of a large amount of saccharine solution, and, growing, will increase a thousand-fold, so a single drop of small-pox virus will, when received into a living body, reproduce many drachms of a fluid endowed with like contagious properties. But no inorganic substance possesses such a faculty, and hence the reproductive power must be ascribed to a living organism, and most probably to a minute vegetable germ, or cell. This view is still further strengthened by the fact of the poison remaining latent in the system for a certain period.

3d. Minute vegetable parasites, or their germs, have been actually observed in connection with abnormal states of living plants, of animals, and of man: By Unger (*Beiträge zür vergleichenden Pathologie*, 1840, p. 1), in the parenchyma of the leaf of chrysomyxa abietes. By many observers, in different species of grain. By Meyen (*Weigmann's Archiv*, 1840, bd. ii. p. 64), on the body of vibriones. By Westwood (*Westwood's Ann. of Nat. Hist.*, 1841, Nov.), on the surface of the silkworm of China. By Busk (*Microscopical Journal*, 1840, vol. i. p. 149), on the surface of dytiscus marginalis. By Bassi (*Del Mal del Segno* v. *Moscardino*, 1837), and Audouin (*Ann. des Sciences Nat.*, t. viii. pp. 229–257), on the surface and in the interior of the common silkworm. By Laurent (*L'Institut*, t. vii. 1839, No. 288), in the albumen and vitelline membrane of the ova of limax agrestis. By Ehrenberg (*Froriep's Neue Not.*, 1839, No. 18), in the salmo eperlanus. By Müller (*Archiv*, 1841, p. 477), Retzius (*Ibid.* 1842, p. 193), and Creplin (*Weigmann's Archiv*, 1842, bd. xiv. p. 61), in pike and other fish; and by Goodsir and Bennett (*Transactions of the Royal Society of Edinburgh*, vol. xv. part 2, 1842), in the cyprinus auratus. By Henle (*Pat. Untersuch*, p. 4), and by Hannover (*Müller's Archiv.*, 1839, p. 338; and 1842, p. 77), on the toes and integument of triton cristatus and punctatus. By Stilling (*Ibid.*, 1841, p. 379), on the surface of frogs. By Rousseau and Serrurier (*Comptes Rendus*, 1841, 5 Juillet), on the surface of testudo Indica, and in the abdomen of hens, doves, and many other birds. By Rayer (*L'Institut*, 1842, No. 450), and by Pappenheim (*Gewebelehre des Anges*, p. 217), in the yolk of hens' eggs. By Müller and Retzius (*Müller's Archiv*, 1842, p. 198), in the respiratory tract of stryx nyctea, and of falco rufus; and by Reinhardt (*Ibid.*, p. 294), on the surface of the lung of various birds. By Ecker (*Verhand der Natur Gesellschaft in Barol*, bd. vii. p. 95), in a closed capsule contained within the abdomen of a raven. By Bennett (*op. cit.*), on the face of a mouse. By Langenbeck (*Froriep's Neue Not.*, 1841, No. 422), in the nasal discharge of a horse with glan-

ders; and by Remak (*Diagnos. und pathog. Untersuch*, p. 225), in the contents of the stomach and intestines of rabbits, oxen, sheep, and pigs.

In man, cryptogamic plants have been found growing on the skin and mucous membranes, both during life as well as after death, and they have also been detected in many of the secretions and excretions of the economy.

On the skin they have been observed by Remak (*Med. Vereinszeit*, 1840, No. 16), Gruby (*Comptes Rendus*, 1841, 12 Juillet and 2 Août), Bennett (*op. cit.*), and others, in favus; by Gruby (*Comptes Rendus*, 1842, Sept. 5), in mentagra and porrigo decalvans (Herpes tonsurans of Cazenave); by Gunsburg (*Pat. Gewebelehre*, bd. ii. p. 30), in trichoma; and by Sluyter (*De Veget. par Ber.* 1847), and Simon (*Hautkrankheiten*, p. 311, 1848), in pityriasis versicolor. On the mucous membrane of the mouth their presence has been signalized in a form of aphthæ (muguet of the French), by Vogel (*Allgem. Zeit. für Chirurg.*, 1841, No. 24), Gruby (*Archiv. Gén.*, 1842, Juin), and others; and by Hannover (*Müller's Archiv*, 1842, p. 285), and Bennett (*op. cit.*), in the coating of the tongue. On the fauces and œsophagus by Hannover (*op. cit.*). On ulcerated spots in the intestines by Langenbeck (*Froriep's Neue Notizen*, 1839, No. 252). In the posterior chamber of the eye by Helmbrecht (*Casper's Wochenschrift*, 1842, p. 593). In tubercular cavities, and in sputa, by Bennett (*op. cit.*) and Rayer (*L'Institut*, 1842, No. 448). In the fibrinous coats of the small bronchial tubes which are expectorated in pneumonia, by Remak (*op. cit.*, p. 222). In vomited matter, by Goodsir (*Edin. Med. and Surg. Journ.*, 1842, April), as well as by Gruby, Vogel, and Busk. In all fluid dejections, no matter how produced, whether by typhus, dysentery, errors in diet, or purgatives, by Remak (*op. cit.*, p. 226), and in the evacuations of cholera patients, by Boehm (*Die Kranke Darmschleimhaut in der Asiatischen Cholera*, 1838, p. 57). In carious teeth, by Erdl. In milk, urine, mucus, and pus, by many observers. We have now to add to the list, their presence in cholera evacuations, where they have been recently again pointed out by Messrs. Brittan and Swayne (*Med. Gaz.*, No. 1139), and in the perspirations of cholera patients, by Dr. Cowdell and Mr. Curme (*ibid.*).

That such vegetable parasites are not unfrequently met with in diseased conditions of animals and man, appears to be unquestionable, and we have now to inquire:—

1st. Whether they or their germs exist in the fixed vehicles of contagions, and in contagious atmosphere.

2d. Whether they are invariably present in contagious diseases, and in those diseases only.

3d. Whether a causal relation obtains between them and the diseased state. The exanthemata frequently arise spontaneously, and still more frequently perhaps they are communicated by contact with the sick, or the air surrounding them. They are produced as well by a something mixed with the air, as by a something existing in, and emanating from, the bodies of the sick. The something mixed with the air, and the something emanating from the sick, may fairly be presumed to be identical. In the exanthemata, the something which possesses the faculty of exciting the same disease in a person previously healthy, is known to be associated with certain fluids of the body—viz. blood, lymph, and pus, for the disease may be communicated by inoculation with those fluids. But the most careful microscopical examinations cannot detect anything differing from normal blood and pus in the blood which, placed beneath the cuticle, causes measles, or the lymph which causes small-pox. Moreover, the constituents of blood and pus cannot traverse the atmosphere, and those substances must therefore act merely as vehicles of contagion. As for the principle of contagion itself, it completely escapes all our means of research, and its material existence cannot be proved at all: still less can it be proved to be a fungus. In the muscardine, a disease which prevails epidemically among silkworms, and which is undoubtedly caused by a fungus, it results, from the researches of Audouin, that the fungus is invariably present, and in every stage of the disease; that it is always one and the same fungus; and that the disease may be communicated to healthy worms by inoculation. This is also the case in favus. Is it so in cholera? are Mr. Brittan's annular bodies always present?

Mr. Brittan himself states that, "in very rapidly fatal cases, these bodies are sometimes to be met with only in very small quantity, or are altogether absent." In the evacuation of a patient admitted to the London Hospital with cholera, which proved fatal in twelve hours, I could not discover these bodies. It does not appear, then, that this fungus is invariably present in cholera evacuations, and it seems particularly suspicious that it should be absent precisely where we should expect to find it in greatest abundance—that is, in the most rapidly fatal cases. I leave for future investigation the solution of the point whether the bodies found in the air, vomit, and dejections are of one and the same nature. Nor do I purpose entering into any discussion as to the precise nature of the bodies themselves, a question which requires a more extended investigation.

Granting them to be, as stated, of a fungoid nature, different species of fungi have been found by Remak, in nearly all forms of fluid dejections, no matter how brought about, and the presence of a fungus, differing, it is true, from that described by Messrs. Brittan and Swayne, in the stools of cholera patients, has been especially noted by Boehm in his work on the morbid states of the intestinal mucous membrane in Asiatic cholera.

3d. Does a causal relation obtain between these fungi and the diseased state? The foregoing considerations lead me to answer this question in the negative. For—1st. The active principle of the *fixed* vehicles of contagion cannot be proved to be a fungus, and therefore analogy would lead me to predicate the same respecting that of contagious atmosphere.

2d. Even were fungi present, they must be proved to be *essential*, and not accidental—the cause of the disease, not its symptom; for fungi are present in various and most different states of the economy, often without giving rise to any particular symptoms.

3d. But these bodies do *not* seem to be invariably present in the evacuations in cholera: they cannot, therefore, be its cause.

4th. The presence of a few fungi does not serve in any way to explain the terrible symptoms of cholera. Nor is our knowledge of the essential nature of this mysterious affection thereby materially advanced.

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54. *Cholera Sporules.*—The following observations, communicated to the *Lond. Med. Gaz.* (Oct. 1849), by Mr. W. R. Basham, tend to show that the cholera sporules cannot be the cause of cholera, as they are not apparent in the cholera discharges until a certain period after the evacuation of these fluids from the body, when they are developed by the spontaneous change which the fluids undergo.

"Desirous," he says, "of examining the cholera sporules from some cholera fluid, I obtained the serous discharges of a male patient, æt. 12, who was collapsed, pulseless, cold, and livid. In less than half an hour, this fluid was under microscopic examination, and the appearances were similar to what I had observed in previous examinations of this fluid—abundance of broken-down epithelium, mucous globules, and disintegrated fibrine, and a few bead-like aggregations of minute crystals of oxalate of lime. The appearance of these crystals in the serous discharges of cholera has been observed also by Dr. Waldo Burnett (*American Journal of Medical Sciences*, July, 1849). I could detect no cholera sporules, after a very patient examination of more than an hour. Four hours after, the appearances in the field of the glass were similar; one or two cholera sporules were now visible, and, as successive drops of fluid were submitted for examination, they became more numerous. Fourteen hours after, similar appearances to those first observed, with the addition of numerous prismatic crystals of the triple phosphate, and the cholera sporules, had become very numerous; they were more abundant in the shreddy membranous portions to which they seemed attached.

Tuesday, Oct. 2d, fourth day.—Fluid in a state of most offensive decomposition; all trace of sporules had disappeared; much shreddy membranous debris, like broken-up and decaying fibrine; millions of vibriones in active motion; nothing else, after a very patient examination of many successive portions of the fluid, could be detected than these animalcules, and the decomposing membranous